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they may give to our country, and the character they may impress upon our people are concealed beneath the mantle of time. These subjects will hereafter afford room for the research of the historian; at this moment they are hidden even from those who almost witness the close of the experiment, and the animating recompense of a patriotic and intelligent people. It is the prerogative of great minds to strike out those improvements, whose excellence, though appreciated at the time, is destined to be fully enjoyed in another age. The *cloaca maxima* is to this day the common sewer of Rome, and the acqueducts of the Campagna still supply to the eternal city, as they did centuries ago, the luxuries and the comforts of life. Their founders have long since ceased to exist, but these proud memorials of their genius remain; and when the statesmen who conceived the project of the Erie and Champlain canals, and that generation who completed them, shall have passed away, the tides of the Hudson will meet in fraternal accord with the waves which wash the shores of Chicago, or beat against the cliffs of Mackinaw:—the most powerful testimony to the force of an enlightened government, supported by a free and educated people.*

Art. XIII.—*Boylston Medical Prize Dissertations for the years* 1819 *and* 1821. *Experiments and observations on the communication between the stomach and urinary organs, and on the propriety of administering medicine by injection into the veins. By E. Hale, jun. M. D. M. M S. S.* Boston, O. Everett and J. W. Ingraham, 1821. pp. 135.

Dr Hale is already very favorably known to the public by a work, which he published some years since, containing a valuable account of the spotted fever, as it prevailed in the town of Gardiner, and by some experiments on respiration, which were communicated to the public in his Inaugural Dissertation. The dissertations at present before us contain an experimental examination of two curious and interesting subjects—one physiological—the other relating more immediately to practical results—and in their design and execution do equal credit to the ingenuity, diligence, and fidelity of their author.

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* Since this article was sent to the press, and of course too late to be of service to us, we have seen a pamphlet, entitled 'the Canal policy of New York.' It consists of a series of letters published sometime since in the *Statesman*, under the signature of Tacitus, and now republished, with an introduction. We observe, moreover, in the reports of the proceedings of congress, that the assembly of Illinois have applied for national assistance toward constructing a canal from the river Illinois to lake Michigan, an enterprize which we have intimated to be practicable.

These dissertations obtained the Boylston premiums for the years 1819 and 1821. These premiums are awarded by a committee appointed by the corporation of Harvard university, and are paid from funds appropriated by the individual whose name they bear, and to whose munificence, public spirit, and love of science, the college owes so many of its finest institutions. They have now been in operation for nearly twenty years, and have been eminently useful in exciting a spirit of emulation and a thirst for literary distinction among the younger members of the medical profession.

The first dissertation is upon a subject which has always excited some interest and much doubt among physiologists, the existence of a nearer passage for fluids from the stomach to the urinary bladder, than that through the circulation and kidneys. We have a thousand familiar examples, in which after taking liquids into the stomach, the quantity of urine is considerably increased within a very short period of time. Now, according to the ordinary course of substances received into the system, and the course which we should *a priori* suppose liquids as well as solids would take, they should be first acted upon by the stomach, if they were capable of undergoing any change from the influence of that organ, sent out into the intestines, absorbed by the lacteals, carried with the chyle into the circulation, and then separated from the circulating mass in the ordinary way by the kidneys. But this seems too long and indirect a process to be so completely effected in so short a time, that the odour or color of substances taken into the stomach shall be perceptible within from fifteen to thirty minutes in the urine: and, accordingly, anatomists and physiologists have at various times been occupied in searching for some other way, by which this operation might be effected.

As has been usual in all inquiries of the kind, a thousand vague conjectures and hypotheses have been broached, some supported by observation, or experiment, others founded upon mere surmise. Of these different opinions, the first part of the dissertation gives us a sufficient account; from which it appears that a great majority of the authors, who have noticed this subject, have believed in the existence of a communication between the stomach and the bladder more direct, than that through the circulation and kidneys, though they differ much in their opinion as to the mode, by which this communication is effected. Indeed, when we examine the facts, the experiments, and the reasoning, which have been applied to the investigation of this subject, it excites little wonder that the truth has not been sooner arrived at.

The first step towards determining, with any certainty, the course which fluids take from the stomach, when they pass with such rapidity into the bladder, seems to have been taken by sir Everard Home. In a paper published in the Philosophical Transactions for 1808, he relates experiments performed on different animals, in which, after tying the pyloric orifice of the stomach, and injecting a fluid into it through the œsophagus, part of the fluid injected was found to have escaped, traces of it were discovered in the spleen, and chemical tests detected it in the urine. He believed, at this time, that the passage from the stomach was effected through the medium of the spleen, but subsequent observation convinced him to the contrary. In a paper published in 1811, he relates experiments, in which, after the extirpation of the spleen, and the tying of the thoracic duct, fluids injected into the stomach were detected in the bile and urine. And his conclusion of course is, that liquids, taken in large quantities into the stomach, do not pass the pylorus, but are taken up in the cardiac portion of that organ by some method, of which we are ignorant, and conveyed into the circulation.

These facts, supposing them sufficiently established by the experiments, tend clearly to show that the fluids taken into the stomach do pass into the circulation, and that the only point in which the course they take to the bladder is shortened, is in being absorbed into the circulation from the stomach, instead of going through the usual operations of that organ, and then being taken up from the intestines, as is the case with the other alimentary substances. Experiments of this kind, however, require to be frequently repeated before full faith can be reposed in the deductions made from them, and it would have been desirable that this should have been done with a view to the present investigation, but we cannot wonder that Dr Hale should have been deterred from their repetition, by their excessive barbarity; and besides, from the experience and character of sir E. Home in this kind of investigation, they can be relied upon with as much confidence, as any single series of experiments ever can be.

The question then to be determined is, whether, having once got the fluids in this summary way into the circulation, they may not in the ordinary course of the functions be found in the bladder after the short period of time in which this is known to take place, without the necessity of supposing any more direct passage. The object of Dr Hale is to show that they may, and in this we conceive he has perfectly succeeded.

With this view it was first necessary 'to ascertain what length of time it would require, for fluids to be carried from the stomach to the bladder, through the medium of the circulation. This can never be ascertained with any degree of precision; yet some approach to the knowledge desired may be made by estimating the rapidity and force of the blood in circulation.' p. 39. Supposing the whole quantity of blood in the body to be thirty pounds, and that thrown out at each contraction of the ventricle, which happens at least seventy times in a minute, to be two ounces; the complete circulation of any one definite portion of blood would be accomplished, according to Dr Hale, in three minutes and twenty-five seconds; and calculating the quantity thrown out at each contraction at only one ounce and a half, the period would be only extended to five minutes and one second. Now, 'the distance from the stomach to the kidneys, through the circulation, is that from the stomach to the heart, through the lungs, back to the left side of the heart, and from the mouth of the aorta to the mouth of the emulgent arteries. This at the most, cannot be supposed to exceed one half of an entire revolution; and is probably less than one fourth. The time requisite, therefore, as far as the circulation is concerned, for fluids to pass from the stomach to the kidneys, is not more than from two to three and a half minutes, leaving the remainder of the time which actually intervenes between their being taken in drink, and ejected in urine for their absorption in the stomach, and their secretion and excretion in the kidneys and bladder.' pp. 40, 41.

This calculation, which is certainly within bounds, shows sufficiently that there is nothing at all improbable in supposing the passage of fluids to be effected through the circulation and kidneys. And this being the natural course, the burden of proof clearly lies upon the other side; and had no question ever existed upon the subject, no further investigation would have been esteemed necessary. Dr Hale has, however, by a few well designed experiments, added new certainty to these conclusions. It is unnecessary to enter into any detail of these experiments; the results to which they lead we shall state in his own words.

'1. The speedy discharge of watery urine after taking a large quantity of liquid in drink, is not occasioned by sympathetic excitement of the urinary organs, but by the actual excretion of the fluid received.

'2. The same portions of fluid, which are received into the stomach, begin, under certain circumstances at least, to be collected in the bladder within twenty minutes from the time when they are taken.

'3. When a large quantity of fluid is taken, the excretion of urine is greatly increased; but the increase does not bear any very exact proportion to the quantity of drink. The increased discharge begins in about twenty minutes, is at its height in about an hour, and terminates, generally, in less than two hours.

'4. When a liquid, which is colored with rhubarb, is taken, the color of the rhubarb appears in the urine; and its appearance in that excretion is not at all confined to the time of the increased discharge, which is occasioned by the quantity of fluids received.

'5. When a liquid, which is of such a nature as to admit of ready detection in the animal fluids, is taken into the stomach, if the quantity is sufficiently large to affect the whole mass of blood, it will be found in the blood drawn from the veins, as soon as in the urine; although it will continue to appear in the urine after it has disappeared from the blood.

'6. When a quantity of coloring matter, sufficient to affect the whole mass of blood is taken, mixed with a large quantity of liquid, the color appears in the urine, as soon, at least, as the excretion *begins* to be increased, and is at the deepest before the increase of urine is at its height; but if the quantity of coloring matter be only a little more than sufficient to tinge the amount of liquids taken, it does not appear in the urine until after a considerable quantity of watery urine has been passed, and does not give its deepest color until some time after the excretion has diminished.' pp. 56—58.

'7. The rapidity, with which fluids pass from the stomach to the bladder, is not increased by increasing their quantity.'

'8. It results from the whole, that the only mode by which fluids received into the stomach can pass into the bladder, is by absorption into the blood, and subsequent secretion by the kidneys.' p. 61.

The second dissertation, upon the propriety of administering medicine by injection into the veins, derives a peculiar interest, from the circumstance of one of the experiments having been performed upon the person of the author. Injections of various substances into the veins and arteries of animals have been frequently practised for the purpose of determining various physiological facts, and sometimes with the view of ascertaining the probable effects of medicines introduced in this way into the system; yet little seems, so far, to have been done to settle the question as to the expediency of the practice. Some instances are related by Dr Hale, in which medicines were injected into the veins, nearly two hundred years ago, to whose efficacy effects truly marvellous were attributed. On these, however, and with good reason, he places but little reliance.

There is a great difference in the effects of foreign substances according to the part of the circulating system into which they are first introduced and the organ to whose vessels they are first distributed. When the injection is made into the carotid artery, so that the first contact is upon the brain, almost every thing, even venous blood and serum, produces instant death. Water alone, or some substance very much diluted with it, can be introduced with impunity. In other parts of the arterial system, however, where the blood is first distributed to other organs of a less delicate structure and a less important function, the consequences are not necessarily fatal. Into the veins, a great variety of substances may be injected without danger to the life of the animal employed, although, generally speaking, some sensible effects are produced by their presence in the circulating system. 'I have injected into the jugular veins of many dogs,' says Bichat, 'bile taken from the gall bladder of other dogs, which I opened at the same time. For the first few days they appeared to be weary, did not eat, were much altered, their eyes were heavy, and they were constantly lying down; but after some time, they gradually regained their former vigor. I afterwards employed human bile in these experiments; the result was the same, except that many times the animal had hiccough and vomiting some time after the injection. In one instance a dog died in three hours after the experiment; but it was because I made use of that extremely black fluid, that is sometimes found in the gall-bladder instead of bile.'

The same experiments were repeated, with the substitution of saliva and nasal mucus suspended in water for the bile, and with similar but less marked effects. Urine, of the strongest kind, was also injected in a number of instances. It was fatal in only one, and that upon the seventh day; but the animals were usually made much sicker by it, than by the other substances employed.

'There is no doubt,' says the same writer, 'that the different substances which can be introduced with the chyle into the blood, may be the cause of various diseases. Is it not the blood which carries to the brain the narcotic principles which produce sleep? Does it not carry turpentine and cantharides to the kidneys, mercury to the salivary glands, &c.? Inject opium, wine, &c. into the veins, and you will stupify the animal, the same as if you had given them by the stomach. Physiologists were, at one time, much engaged with the introduction of medicinal infusions into the veins of living animals. They circulated, by these infusions, purgatives, emetics, and a thousand other foreign substances, the contact of which the blood bore, without occasioning any other accident to the animal, than that of vomiting or alvine evacuations, if they were emetics or purgatives, and a greater or less general derangement, if they were other foreign substances, which had no affinity with any particular organ. The caustics, as the nitric and sulphuric acids, and other very irritating substances, have alone caused death in these curious experiments.'

Of the results of all the various experiments, which have been performed by different individuals, with regard to the injection of fluids into the veins, Dr Hale observes, 'that they are different in different cases, and sometimes give rise to opposite inferences; but the general effect of the whole is to lead to the belief, that the effects of medicinal substances are not materially different, in consequence of the modes of their introduction into the system.'

The experiments of Fontana, for instance, offer different results from those obtained by Brodie, Magendie, and Orfila, in their investigation of the effects of the different poisons, when introduced into the circulation.

Fontana, as quoted by our author, found that the oil or water of the cherry laurel, although a virulent and deadly poison when taken into the stomach, is almost inert when taken into the circulation by a wound or by injection into the veins. Brodie, Magendie, and Orfila, however, found that arsenic or tartar emetic, when injected into the veins, produced nearly the same effects, as when taken into the stomach, except that they were more violent in degree. The opposite results of these different physiologists are, we conceive, attributable to the different nature of the substances employed, Fontana having used a vegetable, and the other individuals mineral poisons in their experiments. The striking differences between the different classes of poisons are sufficiently well known.

The experiments of Dr Hale were performed, with one exception, upon rabbits, and the substances injected were some of the common articles of the materia medica. Comparative trials were instituted, in which the same articles were administered by the mouth, in order to ascertain precisely their relative operation. In the first experiment two rabbits were taken, and two drachms of castor oil administered to each—to one by injection into the vein, to the other in the usual way. On the latter, no sensible effect was produced. In the former, copious operation took place from the bowels, which continued for some hours. A few days after this experiment, the same animals were made the subject of another, in which a drachm and a half of an infusion of rhubarb was injected into the veins of the one which swallowed the oil in the former experiment, and two drachms of the same infusion administered by the mouth to the other, who had previously undergone injection; in the latter, a slight operation took place within the next twenty-four hours—but in the other, no sensible effect was produced.

The sixth, seventh, eighth, and ninth experiments were occupied in ascertaining the effects of medicinal substances when taken into the stomach of rabbits. Six drachms of an infusion of colocynth, containing the strength of about twenty-two grains of that drug—four drachms of an infusion of ipecacuanha of about the same strength—four drachms of an infusion of rhubarb—two scruples of ipecacuanha in powder, were successively given to different subjects, without any operation which could decidedly be pronounced to be produced by the medicine; certainly none upon the stomach. Ten grains of tartar emetic were then administered without any sensible operation, but the next morning the animal was found dead.

In the eleventh experiment twenty grains of calcined magnesia were intended to be injected in five drachms of water. Scarcely half a drachm of the mixture was thrown in before convulsions ensued, and death was produced in less than four minutes. Death followed with equal rapidity after the injection of two drachms of undiluted alcohol, though the same animal had a few moments before borne two drachms of a mixture of equal parts of alcohol and water, without any very serious effects. A drachm and a half of a solution of tartrite of potass and soda was borne without any bad consequences, and seemed to cause a slight operation upon the bowels; but two drachms of the sulphate of magnesia, dissolved in half an ounce of water, caused death before one third of the quantity could be injected. The injections were all made into the jugular vein.

But the most interesting and important experiment was that performed upon his own person; and it is, we believe, the only well authenticated one, in modern times at least, in which any medicinal substance has been injected into the veins of the human subject. The substance selected in this instance was castor oil, and the quantity, half an ounce, a small dose for an adult. This was thrown in at an opening made in one of the veins of the arm, commonly selected for bleeding, and at the temperature of about seventy degrees. The intrepidity, resolution, and perseverance with which this experiment was designed and performed were not a little remarkable; and it was accomplished under circumstances of difficulty and embarrassment, which would have made almost any operator, particularly in his own person, shrink from following up his undertaking. His sensations after the experiment are best detailed in his own words.

'I felt very well for a short time after the operation was finished. The first unusual sensation that I perceived, was a peculiar feeling, or taste of oiliness in the mouth, a little after twelve o'clock,' (about ten minutes after the oil had been completely injected,) 'very soon after, while I was washing the blood from my arm and hands, and was talking in very good spirits, I felt a slight nausea with eructations, and some commotion in the bowels, then a singular and indescribable feeling seemed very suddenly to ascend to my head. At the same instant, I felt a slight stiffness of the muscles of the face and jaw, which cut short my speaking in the middle of a word, accompanied by a bewildered feeling in my head, and a slight faintness. I sat down, and in a few moments recovered myself a little.' pp. 114, 115.

For two hours after the completion of the experiment, Dr Hale continued to experience a strange sensation in his head, a degree of nausea, the oily taste in his mouth, and a tendency to alvine evacuation evinced by wandering pains &c. in his bowels, but without any real operation. The former symptoms left him after this period, but these deceptive feelings in the bowels remained through the evening, though there was no operation at any time, which could be attributed to the oil.

The next day some unpleasant symptoms ensued of a different character.

'I had some pain in the head, and was all day much inclined to chilliness, though without true rigors. My arm was quite painful through the day. I had some fever and loss of appetite, and felt altogether too ill to make any use of my faculties, either of body or mind. This state continued several days; and when I began to recover from it, I found my strength so much diminished, that it required two or three weeks to restore it to its former vigor.' p. 118.

These symptoms, we conceive, however, and we presume Dr Hale to be of the same opinion, are to be in a very considerable degree attributed to the sympathy of the system with the injury to the arm occasioned by the operation, and not to the effect of the presence of the oil in the circulation. They were not different in their nature, nor greater in degree, than might be expected from the violence done to the parts about the vein. And although they would not be necessarily always the consequence of such an operation, yet so great is the probability of their occurrence, as to form an almost insurmountable objection to the adoption of the practice.

But in addition to these unpleasant consequences, there seems to be sufficient ground on other considerations to relinquish the expectation of finding in the injection of medicines into the veins, an advantageous mode of introducing them into the system.

'On a review of the whole subject,' says Dr Hale, 'we find that the evacuations, occasioned by the operation of emetics and cathartics, might be procured quite as effectually, and even more so, by injecting them into the veins, as by introducing them into the stomach; but that it would be dangerous in the extreme to administer in this manner any of the emetics, or of the more power ful cathartics; and that the injection of even the milder cathartics is attended by much more pain and inconvenience, than can be counterbalanced by any advantages that it seems to promise. We must, therefore, regard this mode of administering medicines as too full of dangers to be entitled to confidence.' p. 135.

In this conclusion we perfectly agree, and presume that few, after the experience of Dr Hale, will be inclined to put it again to the test of experiment, either on themselves or others. For all practical purposes, there can be little room for difference of opinion about the expediency of adopting the injection of medicinal substances as a means of curing diseases. Whatever may be the results of this operation upon other animals, they never can inspire sufficient confidence to justify us in making our fellow beings the subjects of such a series of severe and hazardous experiments, as would be necessary in order to establish the practice upon a sure basis; so that even were it determined as a matter of physiological interest, that similar results follow from the injection of medicinal substances, in man as in other animals, we conceive that few would be hardy enough to take upon themselves the risk and responsibility of the operation, in cases of disease.

Dr Hale states very candidly, in his introduction, some of the objections to experiments upon living animals, and observes, with great truth, that 'thousands of animals have died in the cause of physiological science, whose deaths have scarcely added any thing to our knowledge of the laws or properties of the human system.' Now we are not all disposed to deny, that there are questions in physiology which can only be decided by means of experiments on living animals; and such experiments, when performed in the course of a regular train of investigation, may be made to throw much light upon some of the dark points in the science of life. But their necessity and importance has been, we conceive, very much overrated. They have been and are resorted to as modes of proof upon the most trivial occasions, and too often unlimited confidence has been placed upon the deductions drawn from them, when they least deserved it.

Experiments have an imposing air when brought in support of any opinion, and they are too apt to command our implicit belief, without a sufficient examination of their claim to it. Yet surely men are as liable to be mistaken in the results of the experiments they perform, as they are to be wrong in the opinions they hold. Before we give full faith to deductions from experiments made upon living animals, we ought to assure ourselves that the operator is competent to the undertaking. The qualities necessary to it are rarely to be met with; one should have, not only the anatomical knowledge and manual dexterity of the most accomplished surgeon; but a thorough acquaintance with the whole science of physiology, and more especially of the particular subject before him. He should add also to these qualifications a clear, unprejudiced, and philosophical mind, capable of drawing accurate inferences from the phenomena that occur. Yet these circumstances are seldom taken into consideration, and an *experiment* would be received as conclusive from a quarter, whence an *opinion* merely would meet with little regard. But it should be recollected, that there are probably more men qualified to form an *opinion* deserving of attention, than to perform an *experiment;* the latter presupposing even more rare qualities than the former. And, besides, when we decide between the comparative value of opinion and argument on the one hand, and experiment upon the other, we ought to consider that we are more competent judges of the former, than of the latter; we can estimate the weight and importance of men's arguments, and the grounds of their reasoning; but we cannot their powers of observing accurately, or of reporting truly. Hence the remark, we believe of Dr Cullen, that there were more false facts than false theories in medicine, is found abundantly true.

The zeal for matter of fact philosophy, which is so prevalent among some at the present day, makes it necessary that much caution should be used in placing dependence upon its results. This philosophy, when implicitly relied on, is fully as dangerous to the cause of truth as the theoretical, or even more so. Men generally *theorize* upon common and acknowledged facts, so that others can detect the fallacy of their opinions, if they are fallacious; but when they pretend to have opened a new field in nature and to found opinions upon discoveries made by themselves, the evil is more difficult to be remedied, because in such cases we commonly have just as much theory built upon facts, of the evidence for which we are not competent judges. We find, in truth, that experiments, apparently conclusive, are brought in favor of the most opposite opinions; and it would seem as if men formed their opinions first, and then looked about for experiments to support them. It seems impossible, after even the laceration occasioned by the preparation for an experiment, and especially after the lesion or separation of the noble organs which are so frequently the subjects of them, that life should retain enough of the harmony, regularity, and consistency of its operations, for us to judge with certainty of the principles and laws, by which they are governed. Such, it appears to us, must be the disturbance among those functions, whose mutual relation is their mutual support, by so great violence to any of them, that no legitimate inferences can be drawn from the phenomena which occur.

We have only to look at the history of some physiological questions for evidence of the truth of these remarks; in which, after some important facts have been brought to light with regard to them by men of genius, they have been involved in fresh darkness by the accumulation around them of a mass of ill-contrived and ill-digested experiments. Let us not be mistaken; we do not intend to deny, that experiments upon living animals, performed by those competent to the task, with relation to subjects which are capable of being illustrated in this way, and without such injury and laceration of organs as throw into complete disorder all the functions of the system, may be of much service in enlightening some parts of the science of physiology. But when they are performed in the mere wantonness of philosophy, with scarce any other definite object, than that of gaining a name by eliciting some striking phenomena in the struggles of nature, which shall have the air of important discoveries, they are useless at best.*

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* That we do not exaggerate in speaking on this subject, we think will be made evident by the following sketch of some *interesting* experiment on vomiting, performed within a few years at Paris.

M. Magendie, in order to prove that the stomach was inert in the act of vomiting, asserts, that when two fingers were introduced through an incision into the abdomen, the stomach was found not to act itself, but to be compressed between the diaphragm and abdominal muscles; that if the incision was enlarged so as to bring the stomach out of the wound, vomiting ceased, and the stomach remained quiet, although tartar emetic was injected into the veins: that if the abdominal muscles are cut away, vomiting is still produced by the compression of the stomach against the linea alba by the diaphragm, but that if this latter organ be incapacitated by dividing the phrenic nerves, vomiting is at an end. To crown all, M. Magendie removed the stomach, substituted a pig's bladder, which he connected with the œsophagus, and then having sewed up the abdomen, injected tartar emetic into the veins, and succeeded in producing vomiting.

So far all seems very well; but another inquirer, M. Maingault, having taken up the same subject, arrived by experiments performed like Magendie, on living animals, at results directly opposite. He succeeded in producing vomiting, by strangulating an intestine, and by injecting tartar emetic, after dividing the abdominal muscles, cutting them off from the body, dividing the

There are other sources of investigation, from which, if we are willing to resort to them, we can derive nearly all the information and the facts which we need. Nature presents, in the different kinds of animals, the organs and functions so variously constructed and so variously combined, that by tracing out their history accurately through all their varieties, we can come at the truth on most difficult subjects, as well as by having recourse to experiments upon living animals. Nature, in fact, does that which is equivalent to the experiments we perform, by presenting to us, in different classes of animals, the organs in those combinations and under those relations and circumstances, which it is the object of our operations to establish. Thus, it has been doubted by some, whether the bile were formed in the liver, or in the gall-bladder, or in both. How are we to proceed, if we would settle the question by experiment on living animals? We must lay open the abdomen and dissect out the liver, or tie the duct which communicates it with the common biliary canal, or have recourse to some other expedient more ingenious still. How does Haller decide this question? By comparative anatomy. 'It is found,' says he, 'that in many, even large animals, true bile is prepared by a liver alone, without any gall-bladder. That, on the other hand, no animal has a gall-bladder, without having also a liver—and none a gall-bladder so far separated from the liver, but that it is either connected with it directly or communicates with its excretory duct. We conclude then, that a liver, is necessary for the secretion of bile, but that a gall-bladder is not necessary—and that it passes from the liver into the gall-bladder.' Now who will doubt which method of investigation is most likely to give a satisfactory result? We are convinced that many subjects which still remain involved in obscurity, notwithstanding the innumerable experiments made upon living animals in order to illustrate them—and we may mention particularly the functions and relations of the nervous system—might be placed in a much clearer point of view, by throwing upon them the light which may be derived from the careful study of comparative anatomy and general physiology.

phrenic nerves, and even after completely removing the diaphragm itself. *Memoire sur le Vomissement*, 1813.

We know of no way of reconciling conclusions so diametrically opposite, from a course of investigation so similar in each case; and fear there is no other mode of deciding the controversy, but by engaging afresh in the perpetration of these truly edifying experiments.